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ABSTRACT

This study used linked maternal-child data from the 1997-1998 National Longitudinal Survey of Youth to explore the wellbeing of children born to teenage mothers. Two econometric techniques explored the causal impact of early childbearing on subsequent child and adolescent outcomes. First, a fixed-effect, cousin-comparison analysis controlled for unobserved family characteristics that might influence child outcome. Second, outcomes among children born to women who had miscarriages during adolescence were examined. Results found that teen childbearing played only a small, if any, causal role in children's performance on standardized tests, use of marijuana, and fighting. Pre-birth characteristics of teen mothers, birth order, and family size were more important factors in determining these outcomes. For grade repetition, early sexual initiation, and truancy, the fixed effects and miscarriage analyses produced differing results. Teen childbearing had no statistically significant results for any outcomes in the miscarriage analysis. However, results suggested that teen childbearing related to grade repetition, truancy, and possibly early initiation of sexual activity. Results indicate that teenage mothers share more in common with young women who have miscarriages than with their own siblings who delay childbearing. It is those commonalities that drive the zero-order association between early fertility and several negative behavioral consequences for offspring. (Contains 27 references.) (SM)

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The Well-Being of Children Born to Teen Mothers: Multiple Approaches to Assessing the Causal Links

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Abstract

Children born to early-childbearers display high prevalence of problem behaviors and poor academic performance. Previous research indicates that many adverse outcomes stem from poverty or other risk-factors, not from early childbearing *per se*. This paper uses linked maternal-child data from the 1979-98 National Longitudinal Survey of Youth to explore these questions in greater depth.

Using the large sample size made possible through an expanded adolescent sample, we use two econometric techniques to explore the causal impact of early-childbearing on subsequent child and adolescent outcomes. First, we use a fixed-effect, cousin-comparison analysis to control for unobserved family characteristics that may influence child outcomes. Second, we examine outcomes among children born to women who had miscarriages during their teen years. Because teenagers who have miscarriages are in some ways similar to teens who carry infants to live birth, miscarriage data allows us to further scrutinize whether delayed childbearing is associated with improved outcomes.

In both analyses, we find that teen childbearing plays only a small, if any, causal role in children's performance on standardized tests, reported use of marijuana, or fighting. Pre-birth characteristics of teen mothers, birth order, and family size are more important factors in determining this set of outcomes. For other outcomes, namely grade repetition, early sexual initiation, and truancy, the fixed effects and miscarriage analyses produce differing results. Teen childbearing has no sizeable or statistically significant results for any of our outcomes in the miscarriage analysis. However, the fixed effects results suggest teen childbearing is associated with grade retention in school, school truancy, and possibly with early initiation of sexual activity. We interpret these differing results to suggest that teen mothers share more in common with other young women who conceive, but due to miscarriage, do not carry their pregnancies to term than they do with their own siblings who delay childbearing. It is these commonalities that appear to drive the zero-order association between early fertility and several negative behavioral consequences for off-spring. In the paper, we discuss the implications of these findings and possible social policy responses to adolescent parenthood.

Introduction

Teenage parenthood remains one of the most hotly-contested social issues in American society. Debates over the effects of early childbearing rage not only in the popular media and in political discourse, but in the academic literature as well. While much of the public understanding of teen parenthood's impact likely stems from observation of early childbearing's association with a host of negative outcomes, academics have begun to pay careful attention to sorting out the causal role of teen parenting from the effects of background factors that select women into early motherhood. However, the academic literature is far from achieving a firm consensus on the effects of adolescent parenthood. Our knowledge of the effects of early parenting on children born to young mothers is particularly sparse, especially for older children who themselves have reached the teen years.

Although early research tended to support the popular belief that teen parenthood is harmful to both mothers and children, later research has questioned the causal role played by early parenting, and has paid more explicit attention to other family and individual background factors that might account for teen parenthood's apparently detrimental effects. The work of Arline Geronimus and collaborators is perhaps the most prominent body of work that has openly questioned the negative effects attributed to early parenthood (Geronimus and Korenman, 1992; Geronimus and Korenman,1993; Geronimus et al, 1994).

This paper uses recently released, 1979-98 data from the National Longitudinal Survey of Youth (NLSY) to examine a variety of children's outcomes. Our analysis examines psychometric test scores of younger children and retention in school—two key indicators of academic performance. We also examine adolescent fighting, truancy, marijuana use, and early sexual initiation to create a more complete picture of the relationship between maternal age and child behavioral outcomes.

As in our earlier research using the NLSY (Levine, Pollack, and Comfort, 2001), we hope to contribute in four ways to the growing literature on the impact of early parenting on subsequent outcomes among children.

First, we take a thorough approach to assessing the causal impact of teen parenting. As no one method of addressing unmeasured heterogeneity is perfect -- most methods potentially introduce biases of their own -- we have chosen to use three different methods of controlling for unmeasured confounding factors. This triangulation allows us to compare results across methods.

Second, we examine outcomes for adolescents and young adults who were born to teen mothers. Although this group is of clear policy import, it has received less systematic research attention than have younger children within the burgeoning literature on teen parenting.

Third, we examine many diverse behavioral and academic outcomes that might be related to teen parenting. These diverse outcomes are of clear policy importance, but they also reflect the different causal pathways by which early childbearing is correlated with subsequent outcomes for children.

Fourth, we replicate earlier research (Geronimus et al.,1994; Moore, Morrison, and Greene,1997) by examining the correlates of academic test scores. By 1998, the NLSY contains a large sample of older children and young adults. Our increased sample size and new data on older children allows us to explore whether there are important changes in a more recent sample that includes older children.

Outline

In the following sections, we first review the various approaches that have been used to control for background differences that may account for the correlation between teen parenthood

and negative outcomes. We next describe the data and methods we use to study the effects of early childbearing on adolescent and young adult children. We next summarize and discuss our results. We conclude with a discussion of the policy implications of our findings.

Previous Studies of Adolescent Parenthood Effects

Teen parenting--particularly births to unmarried teens—is a central concern for many policymakers, researchers, philanthropists, and the public at large. Teen pregnancy was a critical political issue in public debate over the 1996 welfare reform (Weaver and Armacost, 2000). Indeed, several provisions of welfare reform were directly and explicitly intended to reduce the rate of non-marital childbearing within this age group.

While the policy- and grant-making communities have assumed early childbearing to create or worsen many social problems, academics have been less certain. Empirical research consistently shows a strong correlation between teenage parenting and many negative outcomes. Whether these correlations are actually causal remains unclear. A large literature on the consequences of teen childbearing seeks to unpack these concerns. A central problem in this research is to separate the effects of individual, family, and community characteristics from those of early childbearing itself. Factors that lead some young women to bear children may reflect more fundamental economic and educational disadvantages that also harm children.

More subtly, early childbearing is likely to have different effects on different outcomes. If teen parenting causes economic hardship, childhood outcomes that are especially sensitive to income will show strongest effects. If, however, the dominant impact of early childbearing is on parenting skills and behaviors, one might observe large effects for childhood outcomes that are especially sensitive to parenting skill.

The recent literature on the effects of early childbearing on teen mothers themselves examines educational outcomes, adult economic status and welfare use, family formation, and employment (Corcoran and Kunz 1997; Hoffman, Foster and Furstenberg 1993; Hotz, McElroy and Sanders 1997; Geronimus and Korenman 1992; Grogger and Bronars 1993). Three of these five studies use fixed-effects sibling models. Hotz et al (1997) compare teens who had live births and teens who had miscarriages. Grogger and Bronars (1993) compare women who had twins with women who had singleton births. Most of the studies find that early childbearing plays some causal role in outcomes for mothers themselves. However, these studies produce notably smaller point estimates than did earlier research which included more limited controls for background factors correlated with teen parenting.

The recent literature on the effects of maternal age at first birth on children has examined birth and early health outcomes, psychometric tests, educational outcomes, psychological well-being, home environment, early fertility, delinquency and early behavior problems, incarceration, and adult financial status. (Geronimus and Korenman 1993; Geronimus et al.1994; Grogger 1997; Hardy et al 1997; Haveman, Wolfe, and Peterson 1997; Moore et al. 1997). The two papers by Geronimus and colleagues (1993; 1994) use fixed-effects cousin comparisons. The other papers use explicit controls. Grogger (1997) compares the children born to women when they are teen to children with children born later to mothers who began childbearing as adolescents. (See Hao, Matsueda, and Earnhart 1998 for a similar approach using a fixed-effects sibling comparison). Recent estimates for effects on children are less consistent than recent work on effects on teen mothers. Moore et al (1997) find teen childbearing effects on cognitive tests and home environment; Grogger (1997) finds effects on male incarceration rates; and Haveman et al. (1997) and Hardy et al. (1997) find effects on educational attainment, early fertility, and economic outcomes. However, Geronimus et al. (1994) find no effects on children's cognitive development or home environments. Geronimus

and Korenman (1993) find no negative effects (and a few positive effects) on most of their infant health outcomes.

Review of Methodological Approaches to Establishing Teen Parenthood's Causal Role

As previously discussed, teen parenthood is correlated with many negative outcomes. Whether these correlations represent a truly causal relationship is less certain. Whether and when a woman bears children reflects many unobserved characteristics of mothers and families—characteristics which also influence subsequent social and academic outcomes for their children. Because fertility decisions reflect unobserved heterogeneity within the study population, teen childbearing poses difficult problems for the empirical researcher. Below we outline three methods to address unmeasured heterogeneity.

Explicit controls for family background & maternal traits in regression approach

The most straightforward approach—and one which might be the most robust given econometric difficulties with more advanced techniques—is to estimate a multivariate regression model. This approach does not address unmeasured heterogeneity issues that are the focus of the more elaborate methods. However, it uses the full sample with minimal identifying assumptions. Moreover, it uses a simple conceptual framework that may be more robust to complex data problems or to subtle specification errors.

Omitted variable bias tends to overstate the magnitude of early childbearing effects. Moreover, if including explicit controls renders the teen childbearing variables clinically and statistically insignificant, there is no need for more elaborate approaches designed to reduce upward bias in the estimated effect.

Cousin-Comparisons / Fixed-Effects

In this method, one compares outcomes between children born to sisters who begin childbearing at different time points from each other. Since sisters have the same parents, they share many background characteristics. These shared characteristics include both family factors such as parental education, family income, race, and more difficult to measure factors such as parents' psychological profiles. Other background factors shared by sisters include neighborhood context and other variables. To the degree that sisters share individual traits, these models controll for them as well. However, it is important to note that there are many individual traits that are unlikely to be shared by siblings.

Cousin comparisons effectively control for fixed, unobserved characteristics of extended families. However, cousin models (or sibling models when looking at consequences for teen mothers themselves) have several weaknesses. These models do not explicitly address any particular factor that may be common among sisters. If teen parenthood has no effect net of family background in a cousin comparison, one knows only that family background and not teen parenthood drives the outcomes. The method does not indicate the specific variables that produced the observed effect.

Cousin comparisons also assume that the family environment is uniform across siblings. In fact, however, teen mothers may have different family experiences from their siblings. If teen mothers receive greater parental attention or more limited family educational resources, fixed-effects models may aggravate rather than ameliorate the effects of unobserved family factors. There is some evidence that family investments in specific children reflect parents' sense of children's different abilities, aspirations, and behavioral patterns. (See Daniels et al 1985). Alternatively, early childbearing could occur in reaction to differential treatment in a young mother's family of origin.

In addition to these concerns, cousin comparisons only control for family-of-origin

background factors. They do not control for differences between sisters that may influence outcomes for their own children. Differences in cognitive ability, emotional maturity, and education, are not controlled. If teen parenthood effects persist in a cousin comparison model, investigators still must address the possibility of unmeasured individual heterogeneity before concluding that coefficients represent the unique causal role of teen parenthood.

Cousin comparisons are especially vulnerable to two data limitations. The method requires multiple sisters who are heterogeneous in their fertility timing and who have children in the relevant age ranges. This limitation reduces power even within large, nationally representative surveys. Perhaps more important, this method biases the sample towards large families within both generations.

Cousin comparisons may also be more vulnerable to measurement error. Factors such as family income are imperfectly measured. By eliminating the correlated component within extended families, cousin comparisons aggravate these effects, and can produce downward-biased estimates of poorly measured explanatory variables.

Examining Miscarriages

The most creative approach to addressing unmeasured heterogeneity is used by Hotz, McElroy, and Sanders (1997; 2000). Hotz and his collaborators replicate a natural experiment of delaying childbirth by, in a sense, comparing teen childbearers with women who had miscarriages in their teens. Their premise is that most teenagers who miscarry would otherwise have become teen mothers and thus share with teenage mothers important unmeasured factors associated with early childbearing. However, since the miscarriage delays motherhood, miscarriage is assumed to have no causal effect on the outcomes teen parenthood is frequently suspected to cause.

In order to be able to use information from this natural experiment in a regression format, Hotz et al use first pregnancies that occur during the teen years and end in miscarriage as an instrument for teen childbearing. The two requirements for an instrument are met: 1) first pregnancies as a teen ending in miscarriage are correlated (negatively) with the endogenous variable (teen parenthood) and 2) Hotz et al assume that miscarriages are uncorrelated with outcomes of children born later to women who first have miscarriages.

The main weaknesses of this method concern data quality and sample size. Miscarriages may be under-reported due to the stigma of admitting early pregnancy and ignorance of pregnancies that result in early miscarriages. On the other hand, again due to stigma, some abortions may be reported as miscarriages. Another, more subtle, problem is that some teen pregnancies that resulted in miscarriages would probably have been aborted had they progressed. For this reason, the control group does not exclusively contain women who would have been teen mothers if they did not suffer a miscarriage. In addition, miscarriage may not be a fully random event, meaning there might be differences between teens who miscarry and those who become mothers. (See Hoffman 1998 for elaboration and a critical review of all methods to address unobserved heterogeneity in assessing teen parenthood effects.) Finally, the experience of miscarriage itself may have lasting effects on a young woman's psychological or other characteristics, which could in turn translate into negative outcomes for children she has after the miscarriage. If so, then the use here of miscarriage simply as a marker of selection traits into pregnancy with no causal impact on future outcomes is somewhat problematic.

Independent of under-reporting, miscarriages are infrequent enough, especially during the teen years, to create a sample size problem. Sample size of the control group becomes even smaller because many women who miscarry as teenagers will have subsequent births during the teen years, or will never have subsequent children at all. (Hotz et al. (1995) actually include women who first have a teen miscarriage and then go on to have a teen birth as members of the control group.) While sample size limits analysis of effects on mothers themselves, the same

problems magnify in studying children of NLSY teen mothers. The added requirement that all members of the control group have subsequent children during their post-teen years with available outcome data makes the comparison group too small. Indeed, in our analyses for this paper, we were forced to follow Hotz et al's (1997; 2000) practice of including women who first miscarry but then have births as teens in the miscarriage sample.

National Longitudinal Survey of Youth (NLSY) Sample and Measures

The NLSY follows a national sample of 12,686 young men and women who were between the ages of 14 and 21 in 1979. The respondents were interviewed annually until 1995 when the survey became bi-annual. The NLSY over-sampled African-Americans, Hispanics, and poor whites (although the poor white sample was dropped due to funding constraints after 1988). Children born to the female respondents began to be tracked in 1986 with bi-annual interviews (of mother and child, depending on the child's age) and assessments of psychometric and behavioral factors. In 1994 and 1996 (the most recent available waves), children age 14 and over (officially referred to as the "young adults" by the NLSY) received a separate interview designed to explore issues relevant to older children of the original NLSY sample such as sexual behavior, substance use, pregnancy, school completion, and criminal behavior. It is this group of older children we refer to as the adolescent and young adult children.

Since its 1986 inception, the Mother-Child file has included interviews with 10,919 children of the NLSY females. In 1998 (the latest survey year available), 7,067 of these children were interviewed. Of these, 4,924 were under the age of 14 and were eligible for the psychometric assessments we investigate. The additional 2,143 were at least 14 years old and received the survey questions we analyze about grade repetition, and what we are calling behavioral outcomes (early sexual activity, truancy, smoking tobacco or marijuana, and fighting). Children 14 and over are not given the psychometric assessments.

Dependent Variables

In examining the effects of early motherhood on children, we were interested in diverse outcomes that reflect different components of child and adolescent well-being and social performance. (See Appendix 1 for a detailed description of variables in our analyses.) To explore academic skills, we examine performance on standardized tests of reading comprehension and mathematics. Performance on standardized tests has been the subject of previous research (Geronimus et al,1994; Moore et al, 1997).

We also explore other factors that have received less attention. We examine the linkage between early childhood and grade repetition, since grade repetition is a critical outcome that reflects both academic and behavioral concerns. We include repetition only through the 8th grade to ensure that all adolescent children in our sample have equal exposure to the outcome.

We also examine whether young people report having had sexual intercourse before the age of 17, since early sexual initiation is correlated with teen pregnancy and with other adverse outcomes. As a robustness check, we ran the same analysis with earlier thresholds for sexual initiation and obtained similar results.

Our other dependent variables include whether adolescents report fighting in school or at work, truancy, and use of marijuana during the 12 months prior to the 1998 NLSY interview.

In coding each of the variables, we made judgments about apparent miscodes and extreme values. For example, several cases were coded as never having had sexual intercourse, but as having an age at first intercourse. We gave these cases a missing value on both variables. To avoid biasing the results, our analysis of sexual behavior is restricted to individuals who were 16 years of age or older at the 1998 interview. Children born later have not been exposed to the

full risk of having sex before 16. Missing data limits our sample size in varying amounts for different dependent variables.

Independent Variables

Our main independent variables of interest are dummies for maternal age at first birth. We include whether the mother was 16 or younger, 17-18, 19, or 20-21 when she had her first child. We include ages 20-21 because mothers who gave birth in their early twenties may experience different outcomes from mothers who delayed childbirth until later years. The reference category is whether a mother was over 21 years of age. However, given the structure of the NLSY, the age range of the reference category is truncated. The highest age of first birth in the reference category varies depending on for which sample the particular dependent variable is measured. No mother initiated childbearing after age 35 for the sample given the math and reading tests, or after age 26 for the adolescent and young adult sample.

To control for background factors correlated with maternal age, we also include many other explanatory variables. These include child's age, grandmother's education, mother's revised Armed Forces Qualifying Test (AFQT) score, mother's urban/rural and south/non-south residence at age 14, mother's household structure at age 14, and grandmother's labor force status when the mother was 14. We also include the calendar year of the mother's first birth to capture any cohort effects.

Table 1 contains sample sizes and (weighted) means for all variables that appear in analyses.

Methods

For each variable, we use four approaches to explore the linkage between early childbearing and child or adolescent outcomes.

- For each outcome, we first estimate a sparse regression specification with maternal age at first birth dummies and child age as the only control. This specification presents the unadjusted relationship between early childbearing and subsequent outcomes. For virtually every outcome, this analysis yields a strong association between early childhood and adverse outcomes.
- We then estimate a multivariate regression model that controls for many important background factors such as race, maternal family circumstances at age 14, year of mother's first birth, and maternal AFQT scores. Inclusion of such explicit controls removes the observed association between teen childbearing and several adverse outcomes.
- We then estimate a 2SLS model using teen miscarriage as an instrument for teen childbearing. We restrict this analysis to women who experienced first pregnancies as teens (defined for this analysis as under age 20).
- Finally, we estimate fixed-effect regression models of each outcome. These cousin comparisons provide a useful approach to eliminate potential biases flowing from common but un-modeled features of the original 1979 NLSY households.

The consequences of pregnancy may be different for younger teens. To capture potentially important differences between early and late teen childbearing, we include dummies for several age-at-first-birth categories. To examine whether these estimates were statistically meaningful, we also ran all models with a single teen/non-teen variable coded as 1 if a woman began childbearing before age 20 and 0 otherwise. Results in our first two models were robust across both methods of coding age at first birth. Since we limited our IV model to women who experienced pregnancies as teens (under age 20), we included only two dummy variables for age

at first conception being at age 16 or under and 17-18. We also use a dichotomous measure of teen parenting (under age 20). Our fixed-effect model had lower power than the explicit control approach because all parameters are estimated based upon within-family variations in child and adolescent outcomes. We therefore used a simple dummy variable to distinguish teen mothers from others within the sample.

For the explicit control and fixed effects models, we use linear OLS regressions for continuous dependent variables and multiple logistic regression for dichotomous dependent variables. For the IV model, we use a linear model for all outcomes. In all models, we include only the cases that have complete data on all the independent variables in the models using explicit background controls and data for at least one of the dependent variables. In all cases, we compute robust standard errors that allow for the possibility of unobserved correlation between teen mothers in the original NLSY households. Statistical estimates were computed using the STATA software package for personal computers.

It is important to note that our analyses are of effects on children born at any time to women who had their first live birth while in their teens. We choose this approach because teen motherhood may have deleterious effects on the mother that are long-lasting and that are transmitted to all of her children. Some studies suggest that while teen mothers experience initial disadvantages post-birth, they eventually catch up to their later childbearing peers (Furstenberg, Brooks-Gunn, and Morgan 1987). If so, one might fear that we are understating teen motherhood effects by including children born later to women who had their first births as teens. To address this possibility, we also ran all of our analyses with a different definition of teen motherhood. We based this measure on the age of the mother when she gave birth to the respondent child, regardless of her age at first birth. There were no statistically significant differences in effects of teen parenting across the two different definitions of teen childbearing for any child outcome except for early initiation of sexual activity. The meaning of these results will be briefly discussed in the discussion section. (See Hao, Matsueda, and Earnhart 1998 for an exploration of young children's behavioral differences between children born to women in their teens and children born later to women who initiated childbearing while in their teens.)

Miscarriage Analysis

It is useful here to elaborate on our approach of using miscarriages during the teen years as a proxy for unobserved maternal characteristics that may be associated with adverse outcomes. In particular, we focused on NLSY mothers who experienced a miscarriage prior to their first live birth. We created a dummy variable to indicate whether the mother had a miscarriage or stillbirth prior to her first live birth. Age at miscarriage was then constructed based on 1998 interview data regarding the age and outcome of first pregnancy. For those respondents without 1998 data, or whose outcome of first pregnancy was abortion, data about pregnancy loss before first live birth from the 1982 and 1983 survey administrations was used. This was further augmented by data from the 1984 through 1990 survey administrations on pregnancy outcomes since last interview. Finally, age at miscarriage was set to missing if the miscarriage occurred after the respondent's first live birth.

This procedure resulted in 469 respondents in the dataset and 225 respondents in our sample who had at least one miscarriage before their first live birth. These miscarriages occurred as early as age 12, and as late as age 37 in the full dataset, and age 31 in our sample.

There are 88 respondents in our analysis sample who had a miscarriage before age 20 and before their first live birth. Most women who reported teen miscarriages delivered live infants fairly shortly thereafter. The mean number of years between miscarriage and first live births for these women was less than 3.5 years. Thirty-three of the 88 women went on to become teen parents; while 55 reported a first-birth after their 20th birthday. Sixty-seven of these 88 women

gave birth to at least one child before their 22nd birthday.

A total of 420 off-spring (300 "children"; 120 "young adults") have mothers who had miscarriages as teenagers, prior to their first live birth. Of these, 181 off-spring (91 "children"; 90 "young adults") have mothers whose first pregnancies were in their teen years and ended in miscarriage. Of NLSY women who had miscarriages as teenagers, we find 31 who did not have any children in our analysis dataset.

Results

The Correlation between Timing of Motherhood and Children's Outcomes

Table 2 shows results for a regression specification that controls only for child age. This provides the basis for *unadjusted* comparisons of child outcomes by maternal age. For 5 of the 7 studied outcomes, we find a large and statistically significant relationship between teen childbearing and adverse outcomes. We find smaller differences than we observed in our previous work with 1979-96 data (Levine, Pollack, and Comfort 2001), which may reflect a small diminution of teen childbearing effects. However, our point estimates remain very large and statistically significant. We also find that the children of early teen mothers have worse outcomes than those observed for children whose mothers who initiated childbearing in the late teen years.

[see Table 2}

In the area of academic skill, children born to mothers who began childbearing before age 17 score 6 points lower in mathematics and score 7 points lower in tests of reading comprehension – or roughly about one-third of a standard deviation lower than the reference category for both tests. Children of other teen mothers—and indeed children born to women who initiated childbearing at age 20 or 21--also reported lower mean scores than were obtained from children born to NLSY respondents who waited to have children until after their 22nd birthday.

The last 5 columns of Table 2 show adjusted odds ratios for discrete outcomes. Children born to women who had children before age 17 were substantially more likely to repeat at least one grade in school by the 8th grade (Adjusted odds ratio (AOR) = 3.79). Point estimates were also large for fighting and for having sexual intercourse before age 17.

Teen childbearing had more modest effects for truancy, and marijuana use. Effects on truancy were somewhat larger, yet were only statistically significant for the oldest two categories of teen mothers. Effects were smallest for marijuana use and were not statistically significant.

Controlling for Background and Demographic Characteristics

When we include family background controls, we obtain quite different results. Table 3 shows the accompanying results.

[see Table 3]

The direct effects of teen childbearing on mathematics scores are reduced to below two points and are statistically insignificant. The effect on reading scores is reduced less, when compared to table 2, than the effect on math scores, although there is a substantial reduction. Hence, a large proportion of the unadjusted teen parenting effect appears attributable to maternal

AFQT scores, race, and other background factors. These results for older children confirm earlier findings by Geronimus et al (1994) that teen parenting has little, if any, association with reduced test scores in younger children.

A different pattern emerges for grade repetition and for several behavioral variables. Controlling for many other factors, the children of young teen mothers are substantially more likely to repeat at least one grade in school. (AOR=2.18 for the youngest age at first birth category.) The children of young teen mothers are also more likely to have sexual intercourse before age 17 (AOR=2.90), to report that they fight (AOR=2.30), and to be truant from school (AOR=1.48). Indeed, point estimates for truancy become larger, although not quite significant in our multivariate model.

For academic outcomes, we find that gender, race and ethnicity, maternal AFQT, and birth order all have significant effects on at least two of the three outcomes. Girls are much less likely to repeat grades—possibly reflecting the fact that grade repetition occurs for behavioral, and not merely academic reasons. African-Americans display lower test scores than otherwise comparable whites. Hispanics display lower mathematics scores than whites, but do not display lower reading comprehension scores. Neither group displays significant differences in grade retention when compared with non-Hispanic whites. Maternal AFQT scores are positively associated with all test scores and are negatively associated with grade repetition.

For the behavior models, gender had a large and statistically significant negative effect for marijuana use and fighting. Maternal AFQT scores were a mildly protective factor for fighting, but oddly had a slightly positive (with borderline significance) effect on truancy. Girls were less likely to use marijuana or to report fighting. We found smaller race/ethnicity effects. Controlling for other factors, African-Americans were significantly less likely to report recent marijuana use and Hispanics were more likely to report truancy.

The Importance of Birth Order

In the literature examining the effects of teen motherhood on children, it is standard to include measures of birth order in models. We include birth order in Table 3 because the two studies most closely aligned to our study chose to do so (Geronimus et al. 1994; Moore et al. 1997). As we examined similar outcomes but in older children using later NLSY data, we wanted to use similar specifications to compare our findings with earlier waves of NLSY data. In addition, birth order has strong effects that are empirically and conceptually different from the effects commonly associated with early childbearing.

Although one should be concerned that birth order may be endogenous to childbearing (since teen mothers have more children and therefore are more likely to have a child of higher birth order in the sample), we believe it is appropriate to include these variables separately because they reflect very different family processes that affect child outcomes. Moreover, in our analysis of this dataset, birth order appears to have equally important effects for children of older mothers as it does for the children of mothers who initiated childbearing in the teen years.

While birth order is treated simply as a control in the other studies, its effects on academic outcomes are so powerful that we believe it is important to highlight them. In Table 3, higher-parity children perform significantly worse on both academic tests. Large families (though not birth order itself) had an effect on grade repetition.

Because birth order is arguably endogenous to maternal age at first birth, and because birth order has important effects, we ran the models with birth order removed from the regression specification. The coefficients on all variables marking teen childbearing become statistically significant and more negative in predicting the academic test scores. The effect of very early childbearing (at 16 or earlier) on grade repetition also increases and becomes statistically significant. Even without birth order in the model, inclusion of background

characteristics substantially reduces the effects of early parenting on academic outcomes for children. Birth order does not appear to play the same role in our analysis of behavioral outcomes. High birth order is associated with marijuana use, but is not associated with the other examined behaviors and outcomes.

Miscarriage Analysis

Although our multivariate analysis considered many pertinent factors, unobserved or unmodelled maternal characteristics associated with teen pregnancy might bias our analysis. We examine this possibility by using teen miscarriage as an instrument for teen childbearing. Here, we follow the approach of Hotz (1997; 2000). As mentioned earlier, we restrict the analysis to those who experienced a pregnancy as a teen.

Table 4 shows our results. In our 2SLS models, teen parenthood has no statistically significant effects on any of our outcomes. It is important to note, however, that given the small number of teen miscarriages in the data, we have limited statistical power. Indeed, standard errors in most cases are large. But, even if our results were statistically significant, they would not support the notion that teen parenthood causes negative outcomes in children. The majority of our teen parenthood coefficients, while insignificant, are in the opposite direction as expected and some of them are quite sizeable. For instance, the teen parenthood coefficient of -0.10 in predicting grade repetition represents a reduction in the probability of a child's repeating a grade that is over 50% of 0.179, the base mean level of grade repetition in our sample. We should note that we are using a linear model, which allows for such large coefficients. Teen parenthood coefficients in predicting marijuana, fighting, and truancy represent even larger reductions in probability relative to the base mean. The teen parenthood coefficients in models predicting reading score and sexual activity before 16 represent fairly small changes relative to the base mean. Math score is our only outcome in which the teen parenthood direction is in the expected direction, and it is just under one-third of a standard deviation.

We do not think the large coefficients in the unexpected direction that we find for some outcomes are sufficient evidence (especially given the insignificant coefficients) that teen parenting is a protective factor against these outcomes. However, they do provide suggestive evidence that age-at-first birth is a proxy for many unobserved personal and family characteristics, and caution us against asserting causal teen parenthood effects.

Fixed-Effect Cousin Comparisons

Finally, we use a fixed-effect specification to control for unobserved characteristics that are common to NLSY mothers who were present in the same households in the NLSY initial wave. Such fixed-effects include race, religious background, common childhood experiences among NLSY sample mothers, and other factors. In previous work, cousin-comparisons have yielded much smaller teen parenting effects on children than is typically observed in analyses that do not allow for the possibility of such confounding effects.

Table 5 shows our accompanying results. As in the IV analysis, teen childbearing was not related to lower academic test scores. We did, however, find birth-order and family size effects (some of them large) on both reading and math scores—again highlighting the importance of these variables for child outcomes.

Perhaps surprisingly, however, our fixed-effect specification yielded large and statistically significant teen parenthood coefficients for several behavioral outcomes. The association between teen childbearing and grade repetition was especially striking. Our estimated AOR was 10.91 — over four times the AOR we obtained in our multivariate analysis and much higher than we observed with 1979-96 data in earlier analyses (Levine, Pollack, and Comfort 2001). One possible explanation for these results is that within-family variation in

maternal AFQT scores had a much smaller impact on within-family variation in grade repetition than we estimated in our earlier analysis of Table 3.

Teen childbearing was also associated with markedly higher probabilities of truancy (AOR=3.19) and early sexual activity (AOR=6.96, although this last effect was not quite statistically significant). Given data limitations, standard errors are extremely large. Thus we do not have precise estimates of effects. These findings are noteworthy, however, because they are large point estimates in the expected direction, and therefore provide further, if inconclusive evidence, that early childbearing may influence these important outcomes.

We were surprised to find that early childbearing is associated with lower prevalence of self-reported marijuana use. We hope to provide further scrutiny of these results in our subsequent work.

Are there important group differences?

Given the complexity of running four different models, we have limited our tables here to our overall results for the whole sample. However, whether and to what degree teen parenting effects differ across various groups is an obvious question to ask. In analyses not shown here, we have run our explicit control models separately by race and ethnic group, by gender, and by both. (We do not have enough cases to break the IV and fixed-effect models down by group). We have also run analyses separately by whether the mother was married at the time of her first birth.. For each of these comparisons, we obtained point estimates by running the model separately for each group. We also ran single full-interaction models and compared them to models without interactions using likelihood ratio tests to assess whether differences in point estimates were statistically significant. While some group differences do exist for some outcomes, what is most striking in these analyses is the lack of significant differences in teen parenting effects across groups. Finally, as mentioned earlier in the paper, we have compared analyses using different definitions of teen parenthood – that is, whether a woman was ever a teen mother vs. whether she was a teen mother when she had the respondent child. These comparisons similarly find few differences across definition.

When running models separately by gender, the only outcome for which we find significant differences is for having sex before the age of 16. This gender difference is driven exclusively by teen parenting having a larger effect on African-American women than African-American men. White men and women had roughly equivalent teen parenting effects on age of sexual initiation. We were surprised by this finding, as we expected to also find gender differences for fighting and potentially some of the other behavioral outcomes. Indeed, the point estimate of the teen parenting effect on fighting for white men is much larger than for other groups, but confidence intervals around these estimates are large and overlapping. Thus, we might see some more group differences if we had larger sample sizes.

There were differences between African-Americans and whites as well as Hispanics and whites in teen parenting effects on math and reading scores. For African-Americans vs. whites, there were also differences for truancy and early sexual activity, but with borderline significance.

When comparing the children of women who were married at their first birth with children of those who were not married, the only outcome for which teen parenting effects differed was grade repetition (with borderline significance). Surprisingly, in predicting grade repetition, the odds ratio on teen parenting was larger for the children of women who were married at first birth than the children of those who were not married. This finding does not imply, however, that marital status does not matter in and of itself, only that it does not alter the effects of teen parenting. In another set of analyses, we included a dummy variable for maternal marital status at first birth. The coefficient on this marital status dummy variable was significant and sizeable for most outcomes, which may represent causal or selection effects.

Finally, early sexual activity was the only outcome for which there were differences across definition of teen parenthood. The effects of teen parenting were larger when teen parenting is defined as the age of the mother at the birth of the respondent child, however, the differences were quite small.

Discussion

The goal of this paper has been to scrutinize the causal impact of teen pregnancy using explicit controls, cousin comparisons and miscarriage data to identify potential bias due to unobserved characteristics of teen mothers. Perhaps most importantly, our findings suggest that early childbearing influences different outcomes in different ways. We have fairly strong evidence that teen childbearing does not appear harmful for some important outcomes. For others, our findings are more equivocal, depending on the method of controlling for background factors. However, it should not surprise us that teen parenting might have different effects on different outcomes, given the diverse causal pathways that influence child and adolescent development.

For math scores, teen childbearing does not appear harmful, as results consistently show small and insignificant effects across all three models controlling for background. Similarly, while the coefficient on teen childbearing in predicting reading scores is significant in the explicit control model, it is largely reduced from our child-age as the only control model shown in table 2. Since there is no significant teen parenting effect in either the IV or fixed effects specifications predicting reading scores, we conclude that the zero-order association between early fertility and children's test scores is due primarily to individual and family background factors that differ between early and late childbearers. Although teen childbearing is a powerful *marker* for adverse outcomes, it appears to play little or no causal role in shaping these academic skills. Earlier research (Geronimus et al. 1994) indicated that teen childbearing has no direct impact on the test scores of young children. Our analysis finds the same result in recent data that includes older children.

Our findings, like those of Geronimus and her colleagues, diverge from the findings of Moore et al. (1997), which show effects of early childbearing on academic outcomes using the same 1990 NLSY data as Geronimus et al. (1994). Moore and her colleagues use an explicit control approach similar to the one we use in our table 3. Our different results appear to reflect our inclusion of mothers' AFQT scores as a predictor of children's academic performance. When we remove AFQT from the explicit control model, we too find effects on the academic outcomes. AFQT's strong effects are likely due to a combination of factors. AFQT scores reflect maternal genetic endowment to some degree, but since scores are also affected by environment and education (see Winship & Korenman, 1997), they may capture some of the socioeconomic differences between teens that contribute to selection into early childbearing. Due to assortative mating, scores also partly proxy for paternal genetic and socioeconomic background (Phillips, Brooks-Gunn, Duncan, Klebanov, & Crane, 1998). Since we are interested in the effects of early parenting independent of parental cognitive ability and the environmental factors associated with AFQT scores, we believe it is appropriate to include AFQT in these regressions.

Our findings on test scores suggest that policymakers and the public should respond skeptically to alarmist accounts of the harms associated with teen pregnancy in this area. These findings do not, however, provide reason for complacency in addressing the problems of disadvantaged children. The strong association between maternal AFQT scores and children's academic performance is especially sobering, since teen mothers begin their pregnancies with more limited academic skills than is typical of the broader adolescent population.

Teen parenting does seem to play an important indirect role in academic test scores due to the effect of early childbearing on family size. High birth-order children have lower test scores than are observed among other children. We found especially large family-size and birth-order effects in our fixed-effect specification.

The causal pathways that lead to this result are unclear. Teen mothers who subsequently choose to have large families are a self-selected group. Such selection may lead us to overstate birth-order effects. Even so, the consistent association between family size and academic outcomes suggests that efforts to reduce family size may reap benefits.

Our results for grade retention (which can be seen as a combination of an academic and behavioral outcome) and our behavioral outcomes are mixed across outcomes and, at times, inconsistent across models within outcomes. The most consistent results were for marijuana use. Teen parenthood had no significant effects on this outcome in any of our specifications. Given that we did not even find a relationship in our baseline (child-age controls only) model, we feel confident that teen parenthood has no apparent effect on adolescent off-spring's reported recent use of marijuana. We also find fairly consistent results for fighting. While teen parenthood does have significant, sizeable, positive effects on fighting in tables 2 and 3, both the IV and fixed effects models eliminated this effect. We use this evidence to cautiously suggest that teen parenthood may not have any positive causal impact on adolescent children's fighting.

Our remaining variables -- grade repetition, truancy, and early sexual initiation -- all showed a similar pattern of results to each other. Teen parenthood had positive effects in the baseline, explicit control, and fixed effect models predicting these outcomes. (The odds ratio for teen parenthood in the fixed effect model predicting early sex was very large, but not significant). However, in the IV model, effects were negative and insignificant.

Faced with this inconsistency in our two most sophisticated ways of addressing selection bias -- the IV and fixed effects models -- it is important to think about the differences in these approaches. The fixed effects cousin comparisons control for all factors shared by teen mothers and their sisters who delayed childbearing past the teen years. To the degree that these sisters have the same family background, family-level factors are controlled. The fixed effects model leaves uncontrolled any differences sisters experience growing up (such as parents having more income when one of the sisters is born) and any individual-level differences (other than AFQT score) between the sisters themselves (such as psychological traits, soft-skills, etc.). Conceptually, the IV model in some ways is an improvement in that it controls for all traits – whether they are family- or individual-level – shared by women who get pregnant as teens.

Our inconsistent results across the two approaches suggest that teen mothers share more in common with other women whose first pregnancies occurred as teens and ended in miscarriage than they do with their own sisters. However, given data limitations, particularly the small number of miscarriages in our sample, and the very large effect sizes in the fixed effects models, we are unwilling to conclusively state that teen parenthood has no causal effect on grade repetition, early sexual initiation, or truancy. Instead, we believe our findings provide suggestive evidence of this possibility.

Our finding that, aside from early sexual initiation, there are no differences in teen parenting effects across definition of teen parenthood, also has substantive import. This finding suggests that teen parenting effects either a) represent the effects of selection into teen parenting or b) are due to the initial deleterious effects of a woman's age at first birth. In other words, children born to women when they are teens seem to be no worse off than children born to older mothers who were once teen mothers. Our other group difference analyses suggest that, with the exception of just a few outcomes, when we do see teen parenting effects, they are not driven solely by one demographic group. This finding is particularly important in the case of maternal marital status at first birth, as many assume that the reason teen parenting may have deleterious

effects is because teen mothers are often unmarried mothers. Our findings suggest this assumption is incorrect.

In addition to our substantive results, our analyses shed light on the nature of various methods for capturing unmeasured heterogeneity. While both the cousin comparison and miscarriage analyses are technically appealing, they make great demands on data and these demands are difficult to meet in reality. Even in a large-scale national dataset such as the NLSY, there are not enough sibling (and thus cousin) pairs or instances of miscarriage to estimate models without large standard errors in many cases. Given these data limitations, as well as the short-comings of the methods themselves (which we discussed above), it is difficult or even impossible to produce fully conclusive results. Thus, our approach has been one of triangulation in which we are looking for consistent patterns of results across models. Our three different methods of estimating the effect of teen parenting on child outcomes give us some level of confidence in the pattern of results we find.

Study Limitations

This analysis has several limitations that must be considered in evaluating the results. It does not present a detailed longitudinal picture of the evolving family environment available to children. For example, we expect that changing household composition may play an important role as well as changes in family income over time. Paternal age and involvement, neighborhood and community factors are also of clear importance. A cross-sectional analysis is sufficient to determine whether the children of teen mothers experience adverse outcomes. It is not sufficient to provide a rich causal explanation of any resulting teen parent effects.

While we have good measures for mothers' academic ability, we have less extensive data regarding other parental skills, attitudes, and resources. Young men and women self-select into teen parenting. Although we use three strategies to ameliorate these concerns, neither our fixed-effect specification nor our miscarriage analysis can fully address these concerns.

Our large standard errors on teen parenting in Table 4, reflect the relatively low sample size of our analysis dataset. The use of miscarriage as a natural experiment is therefore limited in this population.

Although the NLSY is a high-quality dataset, it nonetheless contains anomalies, inconsistencies, and mis-reporting. We performed extensive data cleaning to address these issues, but some data quality issues remain. As in most studies of behavior, we rely on potentially unreliable self-reports. The study design also limited available data. We have less extensive data for children born before the initial 1979 survey wave than we do for children born after the survey began.

NLSY study design has a particularly strong impact on our resulting sample of adolescent and young adult children of the original NLSY cohort. Most NLSY children in the adolescent and young adult sample are still relatively young (in the mid-teen years). The adolescents and young adults with available 1998 data are disproportionately born to teenage mothers (often young teenage mothers). In addition, given the age restrictions in the original NLSY sample, no mothers of the adolescent and young adult sample initiated childbearing after the age of 26. The seriousness of this issue is reflected in our descriptive statistics. Mean maternal AFQT scores within the young adult sample were almost 10 points below the mean observed for the child sample.

This data structure has two important implications for our results. First, since the sample of adolescents were born to relatively young mothers, it is not fully representative of the population of similar-aged teens. Second, the reference category in our adolescent models is women who gave birth in their early-to-mid twenties rather than the population of all mothers who delayed childbearing during the teen years. We employ age and period controls to

ameliorate resulting potential biases. However, the age structure appears to be an intrinsic limitation of the NLSY.

Our study contains several other limitations. AFQT is measured after the first birth for a small proportion of women, complicating proper causal interpretation of this variable. Analysis of post-1980 births suggests that this limitation does not alter our point estimates.

Policy Implications

Our varied results highlight the challenges facing policymakers who seek to improve outcomes for teen mothers and their children. Most important outcomes are strongly influenced by pre-birth characteristics of teen mothers. Prior to birth, factors such as AFQT scores are resistant, though not impervious to consistent intervention (Neal and Johnson 1996). Such factors are even more difficult to address following childbirth, when teen mothers face new parenting responsibilities in addition to the need to acquire academic skills.

Much of the recent welfare reform debate concerned whether more stringent eligibility policies and work requirements for teen mothers would, on balance, improve child outcomes for poor children. Because teen parenting appears to have different implications for different dimensions of maternal and child well-being, this broad question appears misplaced. Based on the current results, altered incentives for teen childbearing *per se* may have no impact on children's academic performance. Other characteristics of families and the social institutions that serve them appear to be of greater moment. On the other hand, teen childbearing may play a more central role in promoting childhood problem behaviors.

Our findings are pertinent to evaluating the 1996 welfare reform. This legislation makes two requirements of teen mothers. First, it compels minor teen mothers to live with parents or other responsible adults in order to receive cash aid benefits. The basic goal to provide a more nurturing family environment for both teen mothers and their children is appropriate, though this specific policy may have unintended and undesirable consequences for many families.

Current legislation also requires minor teen mothers to remain in school. Educational interventions for teen mothers have yielded mixed success, however, specialized schools for teen mothers such as Baltimore's Poe School and New Haven's Polly McCabe Center have shown promising results (Furstenberg, Brooks-Gunn, and Morgan, 1987). Given our finding regarding the importance of maternal AFQT scores, any intervention that makes further schooling feasible for young women balancing child rearing with other activities may have positive effects on their children's academic outcomes as well (Seitz and Apfel, 1991; 1993). However, merely compelling teens to attend school does not necessarily increase the feasibility of continued schooling.

Schools for teen mothers often include specialized curricula and services tailored to the unique needs of new parents and thus may improve the physical safety and general well-being of children in other ways. A particularly important finding is that teen mothers attending such specialized schools may have lower rates of subsequent fertility. Because repeat childbearing is a key predictor of adverse child outcomes, this effect of specialized schools would be particularly beneficial.

Perhaps the most promising but difficult policy approach is to conceive more carefully targeted measures to provide more stable home environments for children and families at greatest risk. Young teen mothers, who are likely to have special needs poorly accommodated within existing social service systems, might find such support particularly beneficial (Apfel and Seitz 1991a). While more research is needed on the mediators between teen parenting and behavioral outcomes, policies that enhance young families' home environments without inducing unintended familial disruption may provide the most potent strategy to prevent the child behavioral problems that may accompany early childbearing.

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Appendix 1

Dependent Variables

For Children (14 and under as of end of 1998):

Math Standardized Score – standardized score on the mathematics subscale of the Peabody Individual Achievement Test (PIAT), a widely-used assessment of academic attainment. Though a newer revision of the PIAT exists, the NLSY continues to use the 1968 version, and standardized scores likewise are normed according to the 1968 sample (which had a mean of 100 and a standard deviation of 15). This test was administered to children whose "PPVT age" was 5 or older.[1]

Reading Comprehension Standardized Score – standardized score on the reading comprehension subscale of the PIAT, also normed to the 1968 sample (which had a mean of 100 and a standard deviation of 15). This sub-scale measures a child's comprehension of sentences read silently to him/herself. This test was administered to children whose "PPVT age" was 5 or older, and who had achieved a score of at least 19 on the PIAT Reading Recognition subscale.

For Young Adults (15 or older as of end of 1998):

Grade Repetition – coded as 1 if the young adult respondent reported having repeated a grade up to and including grade 8, and 0 otherwise. Grade 8 was chosen so that all young adults have equal exposure to the risk of grade repetition.

Sex before Age 16 – coded as 1 if the respondent had sexual intercourse before the age of 16 and zero if the respondent has not yet had sex or had sex after the age of 15. Respondents who said they had had first sex before age 10 (of which there were 12) were coded as missing. All respondents not yet 16 by the date of interview were excluded from the analysis. Also, any respondent who gave an age at first intercourse, but had also answered no to a question about whether they had ever had sex, was coded as missing (there were 6 such respondents).

Marijuana – coded as 1 if the respondent reported ever having smoked marijuana in the past year, and 0 otherwise.

Fighting – coded as 1 if the respondent reported having gotten in a fight at work or school in the past year, and 0 otherwise.

Truancy – coded as 1 if the respondent reported having skipped school in the past year, and 0 otherwise.

Independent Variables

[1] A child's "PPVT age" is her/his age in months, rounded up to the next month if the birth date is more than 15 days into the birth month.

Mother's Age at First Birth – In some models, dummies were included for the following categories:

Mother 16 or under at First Birth
Mother 17 or 18 at First Birth
Mother 19 at First Birth
Mother 20 or 21 at First Birth, and
Mother 22 or older at First Birth, which was the reference category used in all analyses.

In other models, we used a single dichotomous measure for mother's age at first birth, coded as:

1 = Mother under age 19 at First Birth
0 = Mother age 19 or over at First Birth

Age at Miscarriage Prior to First Live Birth – a dummy variable indicated if the mother had a miscarriage or stillbirth prior to her first live birth. Age at miscarriage was constructed based on data from the 1998 interview on age and outcome of first pregnancy. For those respondents without 1998 data, or whose outcome of first pregnancy was abortion, data about pregnancy loss before first live birth from the 1982 and 1983 survey administrations was used. This was further augmented by data from the 1984 through 1990 survey administrations on pregnancy outcomes since last interview. Finally, age at miscarriage was set to missing if the miscarriage occurred after the respondent's first live birth.

Teen Miscarriage – This dummy variable was coded as 1 if the age at miscarriage prior to first live birth was below 20 years of age.

Mother's AFQT Score (revised) – (unofficial) score on the Armed Forces Qualification Test (AFQT), as computed from Armed Services Vocational Aptitude Battery (ASVAB) tests taken by mothers in 1980. Scores calculated using the Department of Defense's 1989 revision of calculation procedures were employed.

Mother's background variables:

Several dummy variables described the mother's household characteristics when she was 14 years old, based on questions answered in the 1979 NLSY Youth interview. These include:

Mother's South/Non-South Residence – coded as 1 if the mother's state of residence at age 14 was in the South, and 0 otherwise.

Urban Residence – coded as 1 if the mother lived in an urban area at age 14, and 0 otherwise.

Household Structure – a set of three dummy variables, each coded as 1 if, at age 14, 1) the mother lived with both biological parents, 2) with any biological parent, or 3) in a single or no parent household. Each was coded 0 otherwise.

Grandmother in Labor Force – coded as 1 if the adult female in the household the mother was living in at age 14 was employed for pay outside the home at that time.
Grandmother's Education – a set of three dummy variables describing the grandmother's

educational attainment as of the mother's 1979 interview, coded as 1 if the grandmother's education achievement was 1) did not complete 12th grade, 2) completed 12th grade, or 3) completed 13th grade or higher. Each variable was coded 0 otherwise.

Child's Age -- Dummies were included for the following age categories based on PPVT age (in months) in 1998:
Category 1 -- Child PPVT age in 1998 4 to 35 months
Category 2 -- Child PPVT age in 1998 36 to 59 months
Category 3 -- Child PPVT age in 1998 60 to 79 months
Category 4 -- Child PPVT age in 1998 80 to 98 months
Category 5 -- Child PPVT age in 1998 99 to 113 months
Category 6 -- Child PPVT age in 1998 114 to 129 months
Category 7 -- Child PPVT age in 1998 130 to 144 months
Category 8 – Child PPVT age in 1998 145 to 159 months
Category 9 – Child PPVT age in 1998 160 to 176 months

Race and Ethnicity – Two dummy variables were used, one coded as 1 if the child was African-American, the other coded as 1 if the child was Hispanic; both variables were coded 0 otherwise. These variables were created from and based on the classifications in variable C0005300 in the child file, which in turn was based on information about race/ethnicity obtained from the mother's interview.

Gender – coded as 1 if the child was female, 0 if the child was male.

Birth Order – 3 dummy variables were employed, the first coded as 1 if the child was the mother's first child, the second coded as 1 if the child was the mother's second child, and the third coded as 1 if the child was the mother's third or higher child. Each was coded 0 otherwise.

Number of siblings – the number of children in the child or young adult's family. This variable was constructed by counting the number of child and young adult respondents in the sample with the same mother, as of 1998.

Table 1: Weighted Means (Std. Dev. for Continuous Variables in Parentheses)

Variable	NLSY Child Sample		NLSY Young Adult Sample	
	Sample Size	Weighted Mean	Sample Size	Weighted Mean
Ever repeated a grade in school by 8th grade	----	---	1655	0.18
Had first sexual intercourse before age 16	----	---	1529	0.30
Pregnant Before Age 18			361	0.19
Smoked marijuana in year	----	---	1499	0.13
Got in a fight at work or school in the past year	----	---	1589	0.20
Skipped school in the past year	----	---	1592	0.41
PIAT Raw Math Score 1998	2904	40.79 (17.07)	----	---
PIAT Raw Reading Comprehension Score 1998	2874	39.63 (16.23)	----	---
Young Adult aged 14 at time of interview	----	---	1736	0.10
Young Adult aged 15 at time of interview	----	---	1736	0.21
Young Adult aged 16 at time of interview	----	---	1736	0.19
Young Adult aged 17 at time of interview	----	---	1736	0.17
Young Adult aged 18 at time of interview	----	---	1736	0.15
Young Adult aged 19 at time of interview	----	---	1736	0.10
Young Adult aged 20 or older at time of interview	----	---	1736	0.09
Child PPVT age in 1998 36 to 59 months	2908	0.00	----	----
Child PPVT age in 1998 60 to 79 months	2908	0.16	----	----
Child PPVT age in 1998 80 to 98 months	2908	0.16	----	---
Child PPVT age in 1998 99 to 113 months	2908	0.14	----	---
Child PPVT age in 1998 114 to 129 months	2908	0.14	----	---
Child PPVT age in 1998 130 to 144 months	2908	0.14	----	---
Child PPVT age in 1998 145 to 159 months	2908	0.13	----	---
Child PPVT age in 1998 160 to 176 months	2908	0.12	----	---
Mother's Age at First Birth 16 or under	2908	0.04	1736	0.13
Mother's Age at First Birth 17 or 18	2908	0.08	1736	0.25
Mother's Age at First Birth 19	2908	0.06	1736	0.18
Mother's Age at First Birth 20 or 21	2908	0.14	1736	0.24
Mother's Age at First Birth 22 or older	2908	0.68	1736	0.21
Grandmother Completed Less than 12th Grade	2908	0.33	1736	0.50
Grandmother completed 12th Grade	2908	0.47	1736	0.40
Grandmother Completed more than 12th Grade	2908	0.20	1736	0.10
Mother resident of South at age 14	2908	0.31	1736	0.36
Mother resident in urban area at age 14	2908	0.79	1736	0.75
Grandmother in Labor Force when Mother was 14	2908	0.53	1736	0.52
Mother lived with both biological parents at 14	2908	0.75	1736	0.70
Mother lived with at least one biological parent at 14	2908	0.98	1736	0.97
Mother lived with single or no parent at age 14	2908	0.12	1736	0.14
African-American	2908	0.13	1736	0.20
Hispanic	2908	0.06	1736	0.08
Female	2908	0.48	1736	0.49
First Born Child	2908	0.38	1736	0.61
Second Born Child	2908	0.37	1736	0.28
Third or Higher Born Child	2908	0.25	1736	0.11
Calendar Year of Mother's first birth	2908	84.86	1736	79.44
Mother's AFQT-R score	2908	48.64 (27.51)	1736	39.13 (25.80)
Number of children in family	2908	2.73 (1.21	1736	2.83 (1.21)

Note: Child data weighted by NLSY child sample weights. Young adult data weighted by NLSY young adult weights. A child's "PPVT age" is her/his age in months, rounded up to the next month if the birth date is more than 15 days into the birth month.

Table 2: Regression Results--Child's Age Controlled

Independent Variables	Dependent Variables: Academic: Math	Academic: Reading Comprehension	Academic: Grade Repetition by 8th Grade	Behavioral: Sex≤16	Behavioral: Marijuana	Behavioral: Fighting	Behavioral: Truancy
Mother's Age at 1st Birth							
≤ 16	-5.65*** (0.95)	-6.94*** (0.93)	3.79*** (2.27, 6.33)	3.30*** (1.83, 5.95)	1.31 (0.75, 2.12)	3.02*** (1.89, 4.82)	1.09 (0.74, 1.59)
17-18	-5.39*** (0.72)	-5.90*** (0.73)	2.99*** (1.86, 4.82)	2.14*** (1.25, 3.67)	1.16 (0.75, 1.80)	2.24*** (1.46, 3.46)	1.35* (0.96, 1.89)
19	-3.92*** (0.84)	-4.01*** (0.86)	2.21*** (1.33. 3.68)	1.52 (0.87, 2.65)	0.88 (0.54, 1.44)	2.00*** (1.28, 3.13)	1.53** (1.07, 2.20)
20-21	-3.15*** (0.67)	-3.67*** (0.71)	1.57* (0.94, 2.63)	1.28 (0.73, 2.22)	0.90 (0.57, 1.42)	1.56** (1.00, 2.42)	1.14 (0.80, 1.63)
N	2904	2874	1655	1040	1499	1589	1592
Adjusted R^2	0.65	0.59					
-2 Log Likelihood			1614.04	1321.90	1620.04	1657.00	2081.74

Notes:

1. OLS regression coefficients followed by standard errors in parentheses for continuous outcomes. Odds ratios from logistic regressions followed by confidence intervals in parentheses for dichotomous outcomes.
2. Dummies for child's age included but not shown.
3. Results corrected for family clustering.
4. * = p<.10, ** = p<.05, *** = p<.01

Table 3: Regression Coefficients – Family Background Controlled

Independent Variables	Dependent Variables: Academic: Math	Academic: Reading Comprehension	Academic: Grade Repetition by 8th grade	Behavioral: Sex ≤ 16	Behavioral: Marijuana	Behavioral: Fighting	Behavioral: Truancy
Mother's Age at 1st Birth ≤ 16	-1.59 (1.10)	-3.69*** (1.08)	2.18** (1.15, 4.13)	2.90*** (1.52, 5.53)	1.28 (0.70, 2.35)	2.30*** (1.28, 4.17)	1.48 (0.92, 2.36)
17-18	-1.61* (0.89)	-2.59*** (0.91)	1.89** (1.10, 3.25)	1.92** (1.07, 3.43)	1.13 (0.67, 1.89)	1.81** (1.10, 3.00)	1.69*** (1.14, 2.50)
19	-0.98 (0.86)	-1.28 (0.85)	1.63* (0.94, 2.81)	1.43 (0.80, 2.57)	0.88 (0.52, 1.51)	1.79** (1.11, 2.89)	1.76*** (1.19, 2.59)
20-21	-0.75 (0.69)	-1.30* (0.73)	1.31 (0.7[illegible], 2.27)	1.21 (0.68, 2.15)	0.92 (0.57, 1.50)	1.39 (0.87, 2.23)	1.26 (0.86, 1.83)
African-American	-2.28*** (0.59)	-1.58*** (0.60)	1.12 (0.79, 1.60)	0.90 (0.62, 1.30)	0.56*** (0.39, 0.81)	1.21 (0.85, 1.73)	0.94 (0.71, 1.26)
Hispanic	-1.31** (0.60)	0.08 (0.62)	0.84 (0.56, 1.24)	0.87 (0.57, 1.33)	0.95 (0.65, 1.39)	0.95 (0.64, 1.41)	1.40** (1.01, 1.93)
Female	-0.67* (0.35)	0.67* (0.36)	0.51*** (0.40, 0.66)	0.90 (0.68, 1.19)	0.73** (0.57, 0.93)	0.62*** (0.48, 0.79)	0.94 (0.76, 1.17)
Birth Order 2	-1.83*** (0.48)	-2.99*** (0.52)	1.01 (0.68, 1.51)	0.63* (0.39, 1.00)	0.86 (0.58, 1.26)	0.77 (0.50, 1.18)	0.86 (0.60, 1.23)
Birth Order 3+	-2.18*** (0.77)	-4.51*** (0.86)	1.34 (0.69, 1.60)	0.63 (0.29, 1.39)	0.75 (0.38, 1.50)	0.96 (0.46, 1.97)	0.77 (0.42, 1.42)
# Children of Mother	-0.50** (0.24)	-0.42** (0.21)	1.12** (1.01, 1.25)	0.95 (0.84, 1.06)	0.93 (0.83, 1.05)	0.98 (0.88, 1.10)	0.90 (0.82, 0.98)
Mother's AFQT	0.11*** (0.01)	0.12*** (0.01)	0.98*** (0.97, 0.99)	1.00 (0.99, 1.00)	1.00 (0.99, 1.01)	0.99*** (0.98, 1.00)	1.01* (1.00, 1.01)
Calendar Year of mother's first birth	-0.27 (0.09)	-0.51*** -0.10	1.04 (0.93, 1.17)	0.87* (0.75, 1.00)	0.85 (0.76,0.95)	0.98 (0.87, 1.11)	0.94 (0.85, 1.04)
N	2904	2874	1655	1040	1499	1589	1592
Adjusted R^2	0.70	0.65					
-2 Log Likelihood			1511.08	1300.24	1573.40	1613.12	2040.40

Notes:

1. OLS regression coefficients followed by standard errors in parentheses for continuous outcomes. Odds ratios

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from logistic regressions followed by confidence intervals in parentheses for dichotomous outcomes.
2. Dummies for Child's Age, Grandmother's Education, Mother's South /Non-South Residence, Mother's Urban Residence, Mother's Household Structure, and Grandmother in Labor Force included but not shown. All background characteristics when mother was 14.
3. Results corrected for family clustering.
4. * = p<.10, ** = p<.05, *** = p<.01

Table 4: 2SLS Regression Results– Family Background Controlled, Teen Miscarriage as IV

	Dependent Variables						
	Academic			Behavioral			
Independent Variables	Math	Reading Compre-hension	Grade Repetition by 8th grade	Sex ≤ 16	Marijuana	Fighting	Truancy
Mother's Age at 1st Birth ≤ 19	-0.61 (4.38)	5.15 (4.43)	-0.10 (0.17)	0.02 (0.40)	-0.21 (0.25)	-0.27 (0.24)	-0.32 (0.27)
Age at 1st conception ≤ 16	0.62 (1.86)	-2.63 (1.87)	0.15 (0.11)	0.17 (0.27)	0.16 (0.17)	0.22 (0.15)	0.27 (0.17)
Age at 1st conception =17-18	0.64 (2.14)	-2.88 (2.16)	0.14 (0.11)	0.07 (0.27)	0.15 (0.17)	0.16 (0.16)	0.26 (0.17)
African-American	-1.50* (0.89)	-1.47 (0.91)	0.001 (0.03)	-0.02 (0.05)	-0.12*** (0.04)	0.02 (0.04)	-0.03 (0.04)
Hispanic	-0.71 (1.00)	-0.09 (1.02)	-0.05 (0.04)	-0.03 (0.05)	-0.03 (0.04)	0.01 (0.04)	0.08* (0.04)
Female	-0.42 (0.60)	-0.10 (0.61)	-0.10*** (0.02)	-0.02 (0.03)	-0.06** (0.03)	-0.08*** (0.03)	-0.02 (0.03)
Birth Order 2	-0.69 (1.34)	-3.23** (1.38)	0.02 (0.04)	-0.12** (0.06)	-0.03 (0.04)	-0.04 (0.04)	-0.01 (0.05)
Birth Order 3+	-0.15 (1.53)	-3.95** (1.59)	0.05 (0.06)	-0.13 (0.10)	-0.05 (0.07)	-0.02 (0.08)	-0.04 (0.08)
# Children of Mother	-0.76*** (0.27)	-0.84*** (0.28)	0.03*** (0.01)	-0.01 (0.01)	-0.003 (0.01)	0.001 (0.01)	-0.02* (0.01)
Mother's AFQT score	0.11*** (0.02)	0.15*** (0.02)	-0.003*** (0.0007)	-0.0005 (0.0009)	-0.0009 (0.0007)	-0.002** (0.0008)	0.002* (0.001)
Calendar year of mother's first birth	-0.06 (0.28)	0.09 (0.28)	0.006 (0.01)	-0.04* (0.02)	-0.03** (0.01)	-0.01 (0.01)	-0.017 (0.014)
N Adjusted R^2	1076 0.65	1066 0.59	1252 0.07	846 0.02	1141 0.02	1206 -0.02	1208 0.01

Notes:

1. 2SLS regression coefficients followed by standard errors in parentheses.
3. Dummies for Child's Age, Grandmother's Education, Mother's South /Non-South Residence, Mother's Urban Residence, Mother's Household Structure, and Grandmother in Labor Force included but not shown. All background characteristics when mother was 14.
4. Results corrected for family clustering.
5. * = p<.10, ** = p<.05, *** = p<.01

Table 5: Fixed Effects Regression Results– Family Background Controlled

	Dependent Variables						
	Academic			Behavioral			
Independent Variables	Math	Reading Compre-hension	Grade Repetition by 8th grade	Sex ≤ 16	Marijuana	Fighting	Truancy
Mother's Age at 1st Birth ≤ 19	0.13 (1.68)	-1.77 (1.74)	10.91** (1.23, 96.54)	6.96 (0.59, 80.64)	0.09*** (0.02, 0.56)	0.91 (0.23, 3.56)	3.19* (0.91, 11.25)
Female	-1.13** (0.47)	0.19 (0.48)	0.54** (0.34, 0.88)	1.05 (0.56, 1.95)	0.45*** (0.28, 0.76)	0.53*** (0.33, 0.85)	1.14 (0.77, 1.67)
Birth Order 2	-2.52*** (0.65)	-3.48*** (0.67)	0.61 (0.31, 1.22)	0.82 (0.33, 2.05)	0.82 (0.38, 1.79)	0.91 (0.47, 1.75)	1.22 (0.68, 2.20)
Birth Order 3+	-2.87*** (1.11)	-4.87*** (1.14)	0.65 (0.19, 2.20)	0.88 (0.17, 4.66)	0.87 (0.20, 3.60)	1.03 (0.31, 3.35)	1.15 (0.40, 3.32)
# Children of Mother	-1.07** (0.47)	-1.59*** (0.48)	1.25 (0.79, 1.93)	1.07 (0.55, 2.05)	2.51*** (1.34, 4.76)	0.96 (0.62, 1.51)	1.07 (0.73, 1.58)
Mother's AFQT	0.09*** (0.03)	0.03 (0.03)	1.00 (0.96, 1.04)	0.99 (0.93, 1.05)	0.95* (0.90, 1.01)	0.99 (0.94, 1.03)	0.99 (0.96, 1.03)
Year of mother's first birth	-0.32 (0.20)	-0.53** (0.21)	0.94 (0.66, 1.35)	1.20 (0.63, 2.32)	1.05 (0.66, 1.68)	0.76 (0.52, 1.11)	1.04 (0.75, 1.45)
N	2904	2874	359	216	322	360	504
Overall R^2	0.68	0.61					
-2 Log Likelihood			240.96	138.64	195.54	233.02	329.10

Notes:

1. Fixed effects regression coefficients followed by standard errors in parentheses for continuous outcomes. Odds ratios from fixed effects logistic regressions followed by confidence intervals in parentheses for dichotomous outcomes.
2. Dummies for Child's Age included but not shown.
3. Results corrected for family clustering.
4. * = p<.10, ** = p<.05, *** = p<.01

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